Comment on the proposed conservation of the specific name *Phreatamoeba balamuthi* Chávez, Balamuth & Gong, 1986 (currently *Mastigamoeba balamuthi*; Protista, Pelobiontida)

(Case 3362; see BZN **64**: 213–217)

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The application to conserve the specific name *Phreatamoeba balamuthi* Chávez, Balamuth & Gong, 1986 (currently *Mastigamoeba balamuthi*) for a free-living, polymorphic pelobiont, by suppression of its senior subjective synonym *Mastigamoeba longifilum* Stokes, 1886 will not establish the stability being sought. The references cited in the application point to an unpublished review by Walker and Patterson. Because of the poor taxonomic standards of most descriptions in this genus and the absence of type material, this review concludes that the following taxa are subjective synonyms of *Mastigamoeba longifilum*:

Mastigamoeba balamuthi (Chávez, Balamuth & Gong, 1986) Simpson et al., 1997
Mastigamoeba buetschlii Klebs, 1892 (= *M. lobata* sensu Stein, 1878, non Dujardin, 1841)
Mastigamoeba coprophila Lepşi, 1965
Mastigamoeba euglenoides Skuja, 1939
Mastigamoeba flexuosa Stokes, 1888
Mastigamoeba incerta Lepşi, 1965
Mastigamoeba invertens Klebs, 1892
Mastigamoeba lacustris (Penard, 1909) Lemmermann, 1914
Mastigamoeba limax Moroff, 1904
Mastigamoeba nilensis Wawrik, 1960
Mastigamoeba pachyderma Skuja, 1948
Mastigamoeba paramylon (Frenzel, 1897) Lemmermann, 1914
Mastigamoeba psammobia Larsen & Patterson, 1990
Mastigamoeba radicula Moroff, 1904
Mastigamoeba ramulosa Kent, 1880
Mastigamoeba reptans Stokes, 1890
Mastigamoeba rugulosa Lepşi, 1965
Mastigamoeba sapropelica Lepşi, 1965
Mastigamoeba simplex Skuja, 1939
Mastigamoeba sordis Lepşi, 1965
Mastigamoeba torulosa Skuja, 1956
Mastigella danieli Skvortzov & Noda, 1975

Mastigella polyvacuolata (Moroff, 1904) Goldschmidt, 1907
Mastigella radicula (Moroff, 1904) Goldschmidt, 1907
Mastigina limax (Moroff, 1904) Goldschmidt, 1907
Mastigina paramylon Frenzel, 1897

All but one of these names have priority over *Mastigamoeba balamuthi*, the name that the applicant wishes to conserve. One of these names, *M. ramulosa* Kent, 1880, has priority over *M. longifilum* Stokes, 1886. That is, with the proposed suppression of *Mastigamoeba longifilum* Stokes, 1886, *Mastigamoeba balamuthi* (Chávez, Balamuth & Gong, 1986) would not take priority, but rather priority would go to its oldest available synonym, *Mastigamoeba ramulosa* Kent, 1880, which is the valid name for this pelobiont with or without suppression of *M. longifilum*. An even older name, *Cercomonas lobata* Dujardin, 1841, had temporarily been placed in *Mastigamoeba* by Bütschli (1884) and although this was followed by several authors, e.g., Heider (1886, pp. 19–20), Plenge (1899, pp. 242–243), and Goldschmidt (1907, pp. 86–87), Klebs (1892, pp. 289, 299) concluded that this was erroneous and introduced the name *M. buetschlii* for Bütschli's concept.

Through the availability of *Mastigamoeba balamuthi* in culture the name has secured a relatively strong presence in the contemporary literature (e.g. Hinkel et al., 1994). Its oldest available synonym, *Mastigamoeba ramulosa,* is less frequently but currently still in use (e.g. Prowazek, 1903, p. 197; Cash & Hopkinson, 1905, p. 73; Goldschmidt, 1907, p. 158; Lemmermann, 1913, p. 558; 1914, pp. 33, 35; Valkanov, 1928, p. 420; Seckt, 1933, p. 285; Shen et al., 1990, pp. 331–332; Patrick, 1994, p. 378; Ma et al. 2008, p. 377).

Recommendation

Because of the absence of preservable material with many protists, there has been a tendency to use illustrated specimens as types. This has been suggested by the proponent of the current case (BZN **64**: 214) who validly designated an illustrated specimen of *Mastigamoeba longifilum* Stokes as the lectotype. Such actions will leave considerable uncertainty as to the identity of the taxa. We do not believe designations of specimens from illustrations in historical papers achieve objectivity if alternative unambiguous information is available.

We are convinced that ultimately stability will be best achieved by allowing the principles of priority to stand. Given the absence of original or later designated type material, stability of use of the name *Mastigamoeba ramulosa* Kent, 1880 could be achieved through neotypification based on a hapantotype from the culture of the organism currently under the name of *Mastigamoeba balamuthi*, being the only nominal species among the synonyms for which cultures are available. In our case, the neotype would ideally derive from the protist culture 30984 at ATCC (American Type Culture Collections; Manassas, Virginia) that was deposited under the name *Phreatamoeba balamuthi*, or some part of it (such as some component of the DNA) as type material. That material would then act as an objective reference point for taxonomic purposes. The sequence of 16S (small subunit) ribosomal RNA from this culture was described by Hinkle et al. (1994) and is available at http://www.ncbi.nlm.nih.gov/nuccore/463171. As the only culture of the species available, it would fulfil the requirements of Article 75.3.6. that the neotype came as nearly as practicable from the original type locality despite the fact that the culture originated in Gambia whereas *Mastigamoeba ramulosa* was described from Jersey, Channel Islands. No material closer to the original type locality is currently available. Many morphospecies of protists, this one included, do not seem to have a geographically restricted distribution.

The situation with this species is mirrored in tens of thousands of protist species, in which the species concepts are ambiguous because they rest on optical microscope descriptions and drawings but lack uninterpreted type material. Arguments that lead to the adoption of unambiguous type material are, in our view, more likely to achieve stability than suppression of selected dubious names or reversal of precedence.

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Comment on the proposed conservation of *Haliplanella* Hand, 1956 (Anthozoa, Actinaria) by suppression of *Haliplanella* Treadwell, 1943 (Polychaeta)

(Case 3493; see BZN **66**: 312–316)

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The introduction by Hand (1956) of the genus *Haliplanella* and of the family HALIPLANELLIDAE was exclusively based on the assumed presence of a combination of three types of nematocysts in the acontia. However, two of these belong in reality to one variable type (Den Hartog & Ates, in press). Therefore we (re)assign *Sagartia luciae* Verrill, 1898 to the genus *Diadumene*.

Manuel (1981/1988, p. 134) united the family HALIPLANELLIDAE with the DIADUMENIDAE, but he, although admitting the amastigophore to be a category of p-mastigophore, maintained the genus *Haliplanella* based, apparently, on the fact that 'a well-marked parapet and fosse [are] always present in any state of extension' contrary to *Diadumene*. Stephenson (1935, p. 204) had already considered this to be a species level character, see also Stephenson's (1935, p. 184) generic diagnosis. We doubt if 'smoothing away of the parapet and the fosse' can be a character of taxonomic use. As there are to our knowledge no convincing characters separating *D. luciae* more than specifically from its congener *D. cincta* (cf. Stephenson, 1935, p. 184), there is in our view no basis to maintain the genus *Haliplanella*. The recent proposal to conserve the name *Haliplanella* by Fautin et al. (BZN **66**: 312–316) will serve no purpose.

In respect of *Tricnidactis errans* De Oliveira Pires (1988, p. 514) it is also claimed that three types of nematocysts are present in its acontia, but following Schmidt's (e.g. 1972) nomenclature of the nematocysts, as De Oliveira Pires (1988) clearly intends to, there are only two, i.e. spirulae and penicilli B (in two categories). Species descriptions should not be based on a single isolated character (see also e.g. Stephenson, 1928, p. 106), certainly not on a highly controversial one like the amastigophore. This is exemplified by the description of *T. errans*. We have not been able to study this species, but considering the anatomical details supplied by its author, we think *T. errans* belongs to another family. For a more extensive argumentation, see Den Hartog & Ates (2010).

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Comment on *Raja say* Le Sueur, 1817 (currently *Dasyatis say*; Chondrichthyes, Myliobatiformes, DASYATIDAE)

(Case 3410; see BZN **65**: 119–123; **67**: 66–67)

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The American Fisheries Society and American Society of Ichthyologists and Herpetologists joint Committee on Names of Fishes (CNF) is an august body for which we have the greatest respect. However, we have had occasion to disagree with them, in their various forms, in the past (Williams & Bunkley-Williams, 2003, 2005; Williams & Phelps, 1974). I am afraid that we must also disagree with their comment on the present Case (Lea et al., BZN **67**: 66–67).

The CNF presented information that is clearly correct, which we do not dispute. We do disagree with their interpretation of the 'spirit of the Code.' The purpose of the Commission and the Code is to prevent or resolve confusion. If the blind, strict adherence to the Code that they suggested always worked, then no Commission would be necessary.

With all due respect to the CNF, they apparently failed to see the problem in Case 3410. The temptation to change *say* into *sayi* is an irresistible error trap. This has occurred hundreds of times in the past, is occurring right now, and if something is not changed, will continue to occur forever.

The CNF pointed out that they had changed the spelling from *D. sayi* to *D. say* in the 5th and 6th Editions of their official list of fish names for the last 18 years (actually 20 years, Nelson et al., 2004; Robins et al., 1991) and that *D. say* would again be used in the forthcoming 7th Edition (in preparation). This seems a bit contemptuous of the powers of the Commission to render a decision. Or perhaps suggesting the authority of the CNF supersedes that of the Commission? What the CNF did not mention is that their actions have not stopped all of the misuse of this name.

The CNF suggested that their name change solved this problem. However, any quick survey of the semi-popular (e.g. Galić & Nowell, 2010; Hill, 2001; SEAMAP, 2005, 2009), and even the scientific literature (e.g. Pury et al., 2001, Summers, 2000, Ortiz & Lalana, 2008, and other recent citations listed by Eschmeyer & Frickle, 2010) demonstrate that this is not the case. To understand how an ICZN ruling might solve this problem, we should examine some apparent sources of the problem and their possible solutions.

Obviously and foremost is the legacy of 150 years (1841–1991) of almost exclusive and 'official', e.g. 2nd-4th CNF checklists; Bailey et al., 1960, 1970; Robins et al., 1980, but not mentioned in first checklist (Chute et al., 1948) use of the misspelling *sayi*. This may constitute 'prevailing usage' (Article 33.2.3.1 of the Code) and end this discussion in favour of this spelling. Can 150 or more years of prevailing usage be cancelled out by the CNF in 18 or 20 years? We may have been wrong about adequate prevailing usage (Williams & Bunkley-Williams, BZN **67**: 67). Otherwise, all these misspellings in the literature will continue to cause confusion for some time.

To discern other possible reasons for confusion, we polled ichthyologists [I] and fishery biologists [FB], including some experts in elasmobranch taxonomy, throughout the range of this fish species (eastern USA, Central and South America and the Caribbean) on their opinion of the correct spelling (without their checking references) and asked why they chose the spelling. An astonishing 47 (94%) [22 I, 25 FB] chose the *sayi* spelling. But the point was not a popularity contest, but the reasons for this confusion. Most, 24 [11 I, 13 FB] thought this was the proper formation of the word (which it is). More people will always have knowledge of the usually appropriate ending (*sayi*) than will have knowledge of this particular case (*say*), nor will all bother to check an appropriate recent authority. Only 5, [2 I, 3 FB] remembered this spelling from the literature (see paragraph above). Five [1 I, 4 FB] thought *sayi* sounded right, 4 [1 I, 3 FB] thought it looked right, and 1 [FB] thought it felt right. We interpret this to mean that *sayi* looks more 'scientific' or just looks better in general than *say*. One [I] thought *sayi* made more sense, 1 [I] found it more plausible, and 1 [I] chose it from instinct. Arbitrary choice of the spelling *sayi* fulfills some sort of physiological urge for completeness, correctness, or closure. Two [I] thought *say* was too simple, 1 [I] found it was too short, 1 [I] thought one-syllable names were rare, and 1 [FB] thought *say* was too common a word. No one suggested that *say* was a typographical error for *sayi*, but it will always appear that way to many. Of the 3 who chose the *say* spelling, 1 [I] guessed, and 2 [I, FB] remembered this spelling from the literature. Three respondents excluded themselves from the poll (1 FB was aware of the '*say*/*sayi*' controversy and 2, very sadly serious, FBs refused to choose). Eleven [7 I & 4 FBs] did not respond.

Interestingly, many who responded were members of the two lofty organisations that support the CNF. If the fish specialists [Is & FBs] did not like the *say* spelling, what hope is there to convince everyone else? Obviously, confusion exists, will continue to exist, and can only be resolved by ruling in favour of the *sayi* spelling. People feel justified in violating irrational, unjust, or even incorrect rules and laws. A ruling in favour of *say* is always going to appear irrational and incorrect to many. Even if all scientists follow the ruling, which is unlikely, the general public will continue to be confused. We should at least attempt to transmit science to the public in a useful and clear form devoid of taxonomic traps.

The CNF also stated examples of what would be considered similarly incorrectly formed names (Article 31.1.2; Recommendation 31A), but these are irrelevant to the present case because none of them has generated as much widespread misuse and confusion as *D. say*. Nor is such precedence considered by the current Code (Principle (8) of the 'Introduction': 'never by reference to precedent').

The simplest way to prevent *D. say* being misspelled as *D. sayi* is to change the correct name to *D. sayi*. This would eliminate all confusion, all misspellings, and offend no one save possibly a few purists. If the name *D. say* is retained, confusion will continue.

We close with the concept of the spirit of the Code brought up by the CNF. I hope we are correct in our assumption that the Commission exists to eliminate confusion.

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Comments on the proposed conservation of usage of *Testudo gigantea* Schweigger, 1812 (currently *Geochelone* (*Aldabrachelys*) *gigantea*) (Reptilia, Testudines)

(Case 3463; see BNZ **66**: 34–50, 80–87, 169–186, 274–290, 352–357; **67**: 71–90)

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I am writing in support of Case 3463 to stabilise the name of the Aldabra tortoise. My field of study is conservation genetics, particularly of turtles, so the issues with the nomenclature of the Aldabra tortoise are something I have been aware of. I have read the various arguments put forth on this case and would like to join with those who support the case. We have similar problems with the naming of many species in Australia and so are very aware of the problems caused by having an ever changing background of names to deal with. I hope that you will listen to the strong views in support of this case from the many people who have worked very closely with the species.

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A chance to painlessly resolve the problem of the scientific name of the giant land tortoise inhabiting the island of Aldabra and the Granitic Seychelles was frittered away in the 1980s, when conflicting opinions about it were published in several papers dealing with the geography, taxonomy, phylogeny and nomenclature of giant tortoises of the western Indian Ocean (Bour, 1982; 1984a; 1984b; 1985; 1988; Crumly, 1986; Pritchard, 1986). Three of these papers (Bour, 1984a; Crumly, 1986; Pritchard, 1986) and partially also Bour's (1982) paper, addressed the identity of *Testudo gigantea* Schweigger, 1812, a nominal species whose name has been well established for the tortoise native to Aldabra island since 1881 (Frazier, 2006; BZN **66**: 34–50). Although all three authors of these papers are considered to be experts in testudinid taxonomy, their study, analysis and interpretation of Schweigger's original texts describing his new species named *Testudo gigantea* led to three fairly different conclusions concerning the species' identity. As a consequence of their findings, both Bour (1982; 1984a) and Pritchard (1986) declared that the name *Testudo gigantea* Schweigger, 1812 should not be assigned to the Aldabra tortoise and that the name *Testudo elephantina* Duméril & Bibron, 1835 had to be applied to this species. This action, together with the erecting of the new genus name *Dipsochelys* by Bour (1982) and the revival of the forgotten species name *Testudo dussumieri* Gray, 1831 by Gerlach & Canning (1996), was the starting point of a continuing hustle of nomenclatural changes at both genus and species level, ending up in a flood of various, sometimes grotesque and absurd binominal combinations (see Fritz & Havaš, 2007). Hence, Frazier's designation of a neotype for *Testudo gigantea* Schweigger, 1812 in 2006 was a logical step towards normalisation of this situation.

Taking into account its object to support stability of names, the Code provides effective rules and tools to allow the conservation of long established names in their current usage, including in cases where the present taxonomic concept is different from the original concept when the name was established. We give just two examples to illustrate the effectiveness of these tools. Firstly, Wallin (1977) demonstrated that the type specimen used by Linnaeus for his description of *Testudo geometrica* was in reality an individual of *Geochelone elegans* (Schoepff, 1795). In order to conserve the long used scientific name for the Geometric Tortoise in its current sense and to prevent subsequent nomenclatural confusion, Hoogmoed & Crumly (1984), using Article 73 (c) (i) of the Code (1964), designated a lectotype for this nominal species. Secondly, Bour & Maran (1999) suggested that the young specimen used by Linnaeus to define his concept of *Testudo orbicularis* Linnaeus, 1758, was an individual of *Mauremys leprosa* (Schoepff, 1812). Nevertheless, they accepted a verdict that 'Toutefois, nous n'allons pas modifier une situation nomenclaturale stabilisée depuis plus de 120 ans . . .'. ['However we are not going to modify a nomenclatural situation stabilised for over 120 years . . .']. However, they overlooked the fact that this matter had been explained in detail several years previously (Fritz, 1992) and had been definitively resolved two years later by designation of a neotype for *Testudo orbicularis* Linnaeus, 1758 using Article 75 of the Code (1985) (Fritz, 1994). Despite the fact that the identity of *Testudo orbicularis* Linnaeus, 1758 has been quite clear since 1994, Bour (2009) revived this case again. He concluded from the original description by Linnaeus (1758) that *Testudo orbicularis* was based on a specimen of *Mauremys leprosa*. Bour evidently ignored the existence of a name-bearing type (by neotype designation) for *Testudo orbicularis* Linnaeus, 1758, but he expressed very clearly the attitude 'I do not wish to run counter to the stability of nomenclature'. So, despite his confidence that both original type specimen and description used by Linnaeus did not fit the present taxonomic concept of *Emys orbicularis*, Bour advocated stability of nomenclature.

Were all nomenclatural changes inevitable?

The above-mentioned two examples show the efforts of all authors involved to conserve a long used scientific name, fully in harmony with the text of the Code. Is *Testudo gigantea* Schweigger, 1812 a similar case? At that time when three different opinions about the identity of *Testudo gigantea* appeared, three certainties became clear: (1) The interpretation of Schweigger's description was ambiguous; (2) the type specimen of *Testudo gigantea* was lost or unidentifiable; and (3) *Testudo gigantea* Schweigger, 1812 was the well-established scientific name for the Aldabra tortoise (Bour, 1984a; Crumly, 1986; Pritchard, 1986). Even if one considered that Bour and Pritchard are right and that Schweigger's description does not match the Aldabra tortoise, one cannot agree with their opinion that Schweigger's name should not be applied to this species. Taking into account the above-mentioned three certainties, the spirit of the Code (stability) and the rules provided to sort out such matters, the application of Article 75 of the Code (1964, 1985) and the designation of a neotype would have been the most appropriate actions for protection of the scientific name of such a popular and fascinating creature as the Aldabra tortoise. However, this did not happen and the species name *Testudo elephantina* Duméril & Bibron, 1835 was revived and a new genus name *Dipsochelys* Bour, 1982 was proposed for this species.

Treated as generically distinct since 1978, three genus names for the Aldabra tortoise and its relatives have been applied: *Megalochelys* Fitzinger, 1843 (Obst, 1978), *Aldabrachelys* Loveridge & Williams, 1957 (Bour, 1979) and *Dipsochelys* Bour, 1982 (Bour, 1982). Whereas rejection of the Fitzinger's name *Megalochelys* has been nomenclaturally fully justified (*Megalochelys* Fitzinger, 1843 is junior homonym of *Megalochelys* Falconer & Cautley, 1837), rejection of the name *Aldabrachelys* is another nomenclatural matter. Bour (1982) was right that if *Testudo gigantea* Schweigger, 1812 was the type species of *Aldabrachelys* Loveridge & Williams, 1957 and if *Testudo gigantea* Schweigger, 1812 was *Cylindraspis indica* (Schneider, 1793), then *Aldabrachelys*, independently of its different taxonomic concept, becomes a subjective junior synonym of *Cylindraspis* Fitzinger, 1835. Likewise, Bour (2006) was right that if the holotype of *Testudo gigantea* Schweigger, 1812 was *Chelonoidis denticulata* (Linnaeus, 1766), *Aldabrachelys* becomes a subjective junior synonym of *Chelonoidis* Fitzinger, 1835. No matter how bizarre it sounds, fortunately that is true. Bour's claims were based on the principle of typification that means that every nominal taxon is inseparably linked with its name-bearing type whatever it is. However, to retain the intention of the authors of the name *Aldabrachelys* and its taxonomic delineation, and to support nomenclatural stability, the use of Article 70(a) of the Code (1964) would have been preferable to generation of the new genus name *Dipsochelys* Bour, 1982. It must be noted as well that designation of a neotype and the fixation of the name *Testudo gigantea* Schweigger, 1812 for the Aldabra tortoise as mentioned above would have resolved this matter without need to refer the case to the Commission.

Possible future threats for the stability of nomenclature

The elimination of Frazier's neotype designation for the nominal species *Testudo gigantea* Schweigger, 1812 and its replacement name *Testudo macropus* Schweigger, 1820 might stimulate a series of activities challenging other long established names in their current usage. In addition, this would further threaten the stability of the name of the Aldabra tortoise. As is evident, some professionals do not recognise the claims recently published by Bour (2006) that the holotype of Schweigger's *Testudo gigantea* still exists and that it is a specimen of the South American yellow-footed tortoise *Chelonoidis denticulata* (Linnaeus, 1766). Even if Bour is right, Article 75.6 of the Code (1999) provides a rule enabling the conservation of prevailing usage by a neotype.

With no obligation to refer the case to the Commission, there is also a real possibility to follow up Loveridge and Williams' (1957) intention and to retain the taxonomic concept of their *Aldabrachelys* which was determined by definition ('External naris higher than wide . . .'), range ('Aldabra and Madagascar') and contents ('*gigantea, sumeiri* [sic] and †*grandidieri*') and it is clearly in contradiction with the taxonomic concepts of both *Cylindraspis* Fitzinger, 1835 and *Chelonoidis* Fitzinger, 1835 (Loveridge & Williams, 1957). Application of Article 70.3.2 of the Code (1999) enables the fixation of a new type species and the subsequently decisive settling of the name *Aldabrachelys* Loveridge & Williams, 1957 for the Aldabra tortoise and its related species. Moreover, it gives back to Article 13.1.1 of the Code (1999) its true meaning and seriousness ('every new name published after 1930 must be accompanied by a description or definition that states in words characters that are purported to differentiate the taxon'). Without fixation of the species name *gigantea* for the Aldabra tortoise, *Aldabrachelys dussumieri* would become the valid combination for the Aldabra tortoise, a combination, that is practically unknown and the least common in the literature about the Aldabra tortoise. Frazier's proposal to accept the designation of a neotype for *Testudo gigantea* Schweigger, 1812 will solve this matter well.

If Frazier's proposal is accepted, however, we see no reason to suppress the species name *dussumieri* Gray, 1831 as Frazier asks (BZN **66**: 43). Suppression of the subjective junior synonym is unnecessary because according to the Principle of Priority of the Code (1999) it cannot jeopardise the nomenclatural stability of a well-established valid name that is its subjective senior synonym and that will be fixed by the Commission. Concern that without its suppression, use of the species name *dussumieri* (and *elephantina* as well) for the Aldabra tortoise will continue, is useless, because there is a certainty that it will be, irrespective of the Opinion of the Commission. Moreover, taxonomy and phylogeny of the giant tortoises of the western Indian Ocean is still undergoing research and argument and there are several opinions about the number and classification of the valid taxa and their relationships. The name *Testudo dussumieri* Gray, 1831 has its name-bearing type definitely fixed by lectotype designation (Bour, 2006) and though the provenance and identity of the lectotype show its affiliation with the Aldabra tortoise nowadays (Austin et al., 2003; Bour, 2006), development of new scientific methods in taxonomy and their application, together with the study of old and overlooked documents, might change its present taxonomic concept. Also for this reason, demand for suppression of the name

Testudo dussumieri Gray, 1831 seems to be premature and inconvenient and its status as a subjective junior synonym of *Testudo gigantea* Schweigger, 1812 should be retained.

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(3) Washington Tapia

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As part of the management authority for the wildlife and protected areas of the Galápagos Archipelago, Republic of Ecuador, we would like to clarify some misconceptions recently published in the Bulletin regarding the giant tortoises that are endemic to our islands. Although we understand that the Commission makes its decisions on a case-by-case basis, comments of several people who oppose the conservation of the name *gigantea* for the Aldabra tortoise have included allegations about the situation regarding Galápagos tortoises that need to be corrected. It may seem perfectly clear for some taxonomists to follow repeated changes to the scientific name of a single, charismatic species (Hoogmoed, BZN **66**: 355), but the reality in the field is quite different. Every time a scientific name changes it involves the management authority and other collaborating organisations in a process of actualisation of laws, regulations, norms, documents, customs instructions, educational materials, pamphlets and other literature, signage, and so forth. These changes require significant time and effort from trained personnel. We fully understand that taxonomic knowledge and perceptions change over time (this is research that the Galápagos National Park is now actively involved with) and management authorities must assimilate this new information and make appropriate adjustments. However, when names are changed because of technicalities in a complex nomenclatural code, with no increase in our understanding of the animals involved, it is a different matter because it creates uncertainty about these animals, and it also risks creating frustration at the administrative and political levels that can lose interest in the appropriation of resources needed for conservation and management. For example, although Pritchard (1996) concluded that *elephantopus*, the accustomed name of the Galapagos tortoises, should be changed to *nigra* (with about a dozen subspecies), we have never used *nigra* for any of our education or conservation activities: we have too many other pressing priorities involved in the conservation of the tortoises and their ecosystems. The claims made by a few taxonomists that name changes for the Galápagos tortoise caused no problems for the management authority and were accepted without any concerns (Cheke, BZN **67**: 80; Dubois et al., BZN **67**: 83) are not true. The fact that these people did not know of evidence for such problems does not prove that the problems did not exist. In the case of the Aldabra tortoise there is obviously no consensus among taxonomists on which specific name to use, even though the majority are in favour of conserving *gigantea*. This situation only increases the administrative and conservation problems.

For these reasons we sympathise with the opinions of the management authority of the Republic of Seychelles, the other sovereign state that has endemic giant tortoises. The comments of the Minister of Environment (BZN **66**: 287) and the Seychelles Islands Foundation and its staff and collaborators (BZN **66**: 80, 180–181, 352–354), other wildlife authorities in the region like the Mauritian Wildlife Foundation (BZN **66**: 181), as well as the Chairman and members of the CITES Animals Committee (BZN **66**: 283, 356) are consistent with the problems that we face in Galápagos.

We beseech taxonomists to recognise other priorities that are critical to those involved in protecting the animals and their habitats while they are still alive. Stable names, universally understood and accepted, are a basic need.

(4) Belinda Wright

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The Wildlife Protection Society of India has been actively involved in an enormous variety of wildlife conservation issues for more than 16 years. As we operate in a region of exceptional biological diversity, we are continually dependent on the work of taxonomists and systematists, because the species that are ultimately deemed to require protection and special attention are defined by them. Needless to say, we greatly respect professionals from these disciplines, and would not think of interfering with their work. At the same time, we believe that the conservation of the planet's biological diversity has been widely accepted as a global priority, and this enormous challenge can only be met with full collaboration between different nationals and different professionals.

Having recently lost 'Aditya', an Aldabra tortoise at the Alipore Zoo, Kolkata, that was thought to be 255 years old, we are especially interested in this remarkable species. In this light it is highly disappointing to find a raging, ever-more passionate, debate in which half a dozen taxonomists are pitted against other professionals, including not only taxonomists and systematists, but also conservationists, educators, and administrators. A discussion in a scientific journal has now been tainted with acrimony, impassioned personal comments, and discrediting of people who represent another point of view. It hardly seems appropriate or defensible for taxonomists to disqualify and discredit professionals from other disciplines by making so many cavalier comments. There are several perfectly clear and rational statements by professionals who specialise in the conservation of wildlife, explaining why constantly changing scientific names present profound risks to the conservation of endangered species. These have been submitted by, among others, the Chair and members of the CITES Animals Committee (Althaus BZN **66**: 256–257; Grimm BZN **66**: 283) and professional educators and conservationists from internationally respected organisations (Beaver BZN **66**: 80; Honegger BZN **66**: 276; Mortimer BZN **66**: 285; Rhodin BZN **66**: 87–88; Schmidt BZN **66**: 288–289), not to mention the Minister of Environment of the Seychelles (Morgan BZN **66**: 287) and the agency responsible for the conservation and management of the tortoise in question (Fleischer-Dogley et al. BZN **66**: 180–181). The list even includes respected systematists who have made major scientific contributions on the tortoise whose name is so hotly debated (Arnold BZN **66**: 177). Yet, a few taxonomists (Hoogmoed BZN **66**: 254–256; Cheke BZN **67**: 79–81; Dubois et al. BZN **67**: 82–89) would have us believe that all these people do not know what they are talking about. They say that it is perfectly fine for scientific names to be in a constant state of debate, that this chaos and uncertainty has no relevance to the many complex challenges that are involved in the conservation of endangered wildlife, particularly highly attractive species like the Aldabra tortoise that is continually trafficked internationally on black markets. We at the Wildlife Protection Society of India know very well after decades of unending problems and challenges, particularly those involving illegal trade in wildlife, that the above-mentioned statements of professional conservationists, educators, and administrators are absolutely true and credible. We can only hope that taxonomists will learn to respect other professionals and understand the wisdom of collaborating with them or the consequences will be dire. Disparaging conclusions 'that issues beyond mere science and nomenclature are at issue here' (Cheke, BZN **67**: 81) can only further degrade the discussion. Certainly, the designers of the ICZN website were perfectly aware of the fundamental need for 'stable and universal nomenclature' when they wrote: 'International conventions and national or regional legislation concerning threatened or endangered animals specify the species or subspecies name of the animals that the law intends to protect. Thereafter, protection goes with the name rather than the endangered species itself. Any subsequent change in name could therefore affect conservation measures. The Commission often acts to protect the names of endangered species.' (http://www.iczn.org/Conservation.htm). Hence, the need to stabilise the name '*gigantea*' for the Aldabra tortoise is not about 'personal preference or personal comfort' as was claimed so derogatorily (Iverson BZN **66**: 284; Dubois et al. BZN **67**: 88), but rather about a name that requires recognition by different institutions for their proper function.

(5) Sharon Okanga

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I wish to state my support for case 3463. As a biologist who is naive to the specifics of scientific nomenclature, I used the name *Dipsochelys dussumieri* to describe the Aldabran giant tortoise, in two publications: Okanga & Rebelo (2006) and Wazala & Okanga (2006).

However, having now read the presentation in case 3463, I have been convinced by the sound arguments and have been made aware of my error in judgment. I wish to conserve the name *Testudo gigantea*, and I request that these publications not be used in any case supporting its replacement by *Dipsochelys dussumieri*.

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(6) Ferry Grunewald

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With awe I have read the whole of Case 3463. I am quite shocked to see how this case has developed into a schoolyard brawl. Especially the interference of people who don't normally work with the Aldabran Giant tortoise is quite disturbing. Also the fact that scientists let themselves be driven by personal emotion without a scientific reason upsets me. Yet, because this has happened I feel obliged to give my point of view as well. I consider myself just a hobbyist/layperson. Yet I have read the Code. I have also read the works of both Bour and Frazier. Using the Code, one can come to only one conclusion and that is that Bour's argument is the correct one. I am aware that outside Europe many people continue to use the name *Aldabrachelys* or *Geochelone gigantea*. I believe this is due to a lack of information about the articles of Bour. Yet, on the European continent, the name *Dipsochelys dussumieri* is probably the most used name for the Aldabran Giant Tortoise. The editors of TRIONYX for example have been using this name since 2003 instead of *gigantea*. The articles of Bour are favourites among European turtle keepers and interesting as they show that Bour does his research very thoroughly, diving into the past of many species of turtles, unearthing their true history.

Objections of conservationists worldwide that changing the name to *Dipsochelys dussumieri* would interfere with their work and conservation of this tortoise are in my opinion unfounded. Names can be changed in documents virtually overnight. More importantly, the world's leading conservationist in the field of the Aldabran Giant tortoise, Justin Gerlach, has been using the name *Dipsochelys dussumieri* for a decade now. His 2004 book has become somewhat of an icon as well.

I personally saw and measured the type of *Dipsochelys dussumieri*, RMNH 3231, in the Leiden Museum. There is no doubt about its identity. It looks like a perfect juvenile of the Aldabran Giant tortoise. I too believe that its name, based on the rules of the Code, should be *Dipsochelys dussumieri*. I recommend that the Commission rejects Frazier's proposal. This case has already gone way too far.

(7) Scot Ramsay

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In a paper I published some years ago (Ramsay et al., 2002) I referred to the species as *Dipsochelys dussumieri*. I would like to clarify that my publication was not meant as validation or particular support for *dussumieri* over *gigantea*. Further, having looked at the case information, I agree with Case 3463, and offer my support for conservation of the name *Testudo gigantea*.

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Comment on the proposed conservation of usage of *Allosaurus* Marsh, 1877 (Dinosauria, Theropoda) by designation of a neotype for its type species *Allosaurus fragilis* Marsh, 1877

(Case 3506; see BNZ **67**: 53–56)

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As the state paleontologist of Utah, I heartily support the proposal to make USNM 4734 the neotype of *Allosaurus*. *Allosaurus* has been the Official State Fossil of Utah for many years based primarily on the 54+ specimens of *Allosaurus fragilis* from our Cleveland/Lloyd Quarry (a national historic landmark) with many skeletons exhibited across the globe. No large theropod dinosaur is as well documented as *Allosaurus* and to risk losing this name would only serve to confuse and complicate our science among the people of the world. The logic presented in Case 3506 is simple and straightforward and will provide for greater stability in systematics.

Comments on the proposed conservation of usage of *Archaeopteryx lithographica* von Meyer, 1861 (Aves) by designation of a neotype

(Case 3390; see BZN **64**: 182–184, 261–262; **65**: 314–317; **66**: 87–88, 357–358; **67**: 90–93)

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I disagree with Padian's comment (BZN **66**: 357–358) regarding the proposed neotype designation for *Archaeopteryx lithographica* von Meyer, 1861. It is appropriate to designate the London specimen (BMNH 37001, the Natural History Museum) as the neotype of this taxon. Contrary to Padian's suggestion, the Commission has a chance here to make a nomenclatural situation clearer, resolving a long-standing issue that surrounds one of the most famous fossils. A neotype will allow meaningful comparisions to be made with a specimen that we know is *Archaeopteryx lithographica*: to tread water, as Padian recommends, would be to waste an opportunity. Vertebrate palaeontologists have for years effectively treated the London specimen as the type of *Archaeopteryx lithographica* von Meyer, 1861, and it will almost certainly never be possible to confidently determine whether the isolated feather (the current holotype) really belongs to the same animal as the skeletal specimens of *Archaeopteryx*. In my view, Padian's argument regarding the current absence of any other feathered theropods in Solnhofen does not hold up; the absence of evidence is not evidence for anything.

Comments on the proposed conservation of *Anthochaera* Vigors & Horsfield, 1827 and *Philesturnus* Geoffroy Saint-Hilaire, 1832 (Aves) by suppression of the generic name *Creadion* Vieillot, 1816

(Case 3499; see BNZ **66**: 332–339; **67**: 93–94)

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Gregory & David's comments (BZN **67**: 93–94) on a potential type species designation of *Sturnus carunculatus* Gmelin for *Creadion* Vieillot by Vigors & Horsfield (1827) have led us to review the issue. Gregory & David raise two questions. First, is the 'designation' sufficiently explicit to satisfy Article 67.5? And secondly, how is it affected by Article 69.4, the 'fixation by elimination' rule? Gregory & David concluded that the designation was sufficient, based on Vigors & Horsfield's statement 'leaving the *Sturnus caruncutatus* . . . to represent that group', where group = *Creadion*. They contended that the word 'represent' in this context was an equivalent term for a type or type species (Article 69.1.1). In Case 3499, we (BZN **66**: 332–339) had nevertheless interpreted both clause and 'represent' to mean that *Sturnus caruncutatus* Gmelin was cited merely as an example or representative of *Creadion*, and so not an explicit designation under Article 67.5 (cf. 67.5.1).

As Gregory & David rightly point out, the context in which the term 'represent' was used by Vigors & Horsfield is crucial to its meaning. Accordingly, we reproduce here the *complete* contextual tract from Vigors & Horsfield's discussion of the Australian honeyeater, *Creadion*, so that Commissioners can judge the issue for themselves: 'M. Vieillot has referred this bird to a new genus of his, which he names *Creadion*, and which he divides into two sections; one represented by the *Sturnus caruncutatus*, Gmel., the other by the bird before us. He places this genus in the vicinity of the *Sturnidae*; uniting it with one or two other forms, which are chiefly distinguished by fleshy processes from their heads, into a family under the name of *Caruncutati*. As we consider our present group to have no relation to the *Sturnidae* beyond the unimportant analogical one of having in common with two or three species carunculated appendages to the head, we have no hesitation in removing it from the genus *Creadion*; leaving the *Sturnus caruncutatus* (with which species however we must confess that we are unacquainted) to represent that group.'

Here we make two points. First, Vieillot (1817) did not name his sections of *Creadion*. Secondly, Vigors & Horsfield use the word 'represent' a second time in their discussion, in the first sentence, and apply it there it to the same species in the same way as in their last sentence – also, we would contend, as an example.

Concerning the 'fixation by elimination' rule, we happily agree with Gregory & David's implication that it cannot apply to type species already properly designated. Rather, we see it as complementing Article 67.5 by limiting further the circumstances under which 'loose' or improper designations can be attributed, in the interests of ensuring that designations are rigidly explicit. Given that Vigors & Horsfield's referral of *Sturnus caruncutatus* Gmelin to *Creadion* is such a case, we find it appropriate to invoke the rule (Article 69.40) which also disqualifies Vigors & Horsfield's action as a type designation. We add that this interpretation was originally communicated to us by Dr W.D.L. Ride, former Commissioner; he chaired the editorial committee for the third and fourth editions of the Code, the editions in which the 'fixation by elimination' rule first appeared. We have followed his lead in the belief that if anyone would understand the intentions of that rule and the circumstances in which it should be applied, it should be him. We also take this opportunity to correct misleading wording in our original application, Case 3499 (BZN **66**: 333): in paragraph no. 3, line 12, replace 'is qualified' by 'qualifies'.

At the root of this dispute is the meaning of 'an equivalent term' for a type species in Article 69.1.1. We would interpret it strictly, to such terms as 'the specific name bearer for this genus'; others obviously read it more loosely, bringing it into conflict with the provisions of Article 67.5. In giving the opinions justifiably sought on this case by Gregory & David, could we suggest that the Commission might also consider defining its interpretation of 'an equivalent term', and incorporate that interpretation, preferably with examples, in the next edition of the Code.

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I would like to support the application by Bock et al. (BZN **66**: 332–339) to conserve the generic names *Anthochaera* Vigors & Horsfield, 1827 and *Philesturnus* Geoffroy Saint-Hilaire, 1832. The complex history of the case and the rationale behind the application have been convincingly explained by Bock et al. (BZN **66**: 332–339). However, their remark (p. 334) that *Creadion* Vieillot was employed for Australian wattlebirds only by Mathews (until 1946) is not literally true. Stresemann (1953, p. 84), in his paper on birds collected by Labillardière, deliberately used the combination *Creadion paradoxus* (Daudin, 1800) as a valid name, quoting Mathews (1922, p. 170) and referring to the type specimen in the Muséum d'Histoire Naturelle in Paris. This additional evidence may serve as an example to stress the necessity of conserving the generic name *Anthochaera* Vigors & Horsfield, 1827 for Australian wattlebirds (MELIPHAGIDAE).

Suppression of the generic name *Creadion* Vieillot, 1816 under Article 23.9.3. of the Code seems necessary because neither the strict application of the Principle of Priority for the generic name *Creadion* Vieillot, 1816 nor acceptance of the interpretation by Amadon (1962, p. 158) would be likely to maintain stability in zoological nomenclature. The first would threaten prevailing usage of the generic name *Anthochaera* Vigors & Horsfield, 1827; the second would be inconsistent with the regulation in Article 69.1.1. of the Code. I therefore fully support the application.

Additional references

Stresemann, E. 1953. Vögel, gesammelt von Labillardière während der "Voyage à la recherche de Lapérouse" (1791–1794). *Mitteilungen aus dem Zoologischen Museum in Berlin*, **29**(1): 75–106.

Comment on the proposed conservation of usage of *Mastodon waringi* Holland, 1920 (currently *Haplomastodon waringi*; Mammalia, Proboscidea) by designation of a neotype

(Case 3480; see BZN **66**: 164–167, 358–359; **67**: 96)

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Ferretti (BZN **66**: 358–359) agrees with Lucas (Case 3480, BZN **66**: 164–167) that: (1) *Mastodon waringi* Holland, 1920 has been long and widely used as the valid, type species of *Haplomastodon*, although *Masthodon chimborazi* Proaño, 1922 is the type species of *Haplomastodon* by original designation; (2) *M. waringi* is based on an undiagnostic holotype, so it is a nomen dubium; and (3) the diagnostic parts of the holotype of *M. chimborazi* (the skull and lower jaw) were destroyed in a fire in 1929. Ficcarelli et al. (1995) also recognised these facts, so they proposed a diagnostic neotype for *M. chimborazi* and advocated recognising it as the oldest valid name for a species of *Haplomastodon.*

In a petition to the Commission, Lucas (Case 3480) sought to preserve long and nearly universal usage by proposing a diagnostic neotype for *Mastodon waringi* so that it could continue to be used as the oldest valid name of a species of *Haplomastodon.* Note that only the handful of references cited by Ferretti (BZN **66**: 358–359) use *H. chimborazi* instead of *H. waringi.* Ferretti proposes to continue to recognise *H. chimborazi* based on the parts of the holotype skeleton (two humeri and possibly the atlas vertebra) that are still extant. Nevertheless, these are not diagnostic elements of a species of *Haplomastodon* or of the genus; for example, see Hoffstetter (1950, 1952), who makes it clear that the diagnosis of *H. chimborazi* is based on cranial and dental features, not on features of the postcrania. Furthermore, although Hoffstetter (1952) believed that the presence of a closed transverse foramen in the atlas vertebra is diagnostic of *Haplomastodon*, Simpson & Paula Couto (1957, p. 167) concluded that this feature was 'an individual variation' and thus of no taxonomic significance. Therefore, Ferretti's suggestion is not only to continue to use *H. chimborazi* without a diagnostic holotype (or neotype) but also to overturn wide and nearly universal usage of *H. waringi.* Instead, I prefer to promote the stability and universality of nomenclature by establishing a diagnostic neotype in order to preserve usage of *H. waringi* as the oldest valid name of a species of *Haplomastodon.*